Salt Flats

United States Department of the Interior

BUREAU OF LAND MANAGEMENT
Salt Lake District Office
2370 South 2300 West
Salt Lake City, Utah 84119

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(UT-022)

MAR 14 1994

Dr. Dianne Nielson
Director, Utah Department of Environmental Quality
168 North 1950 West
P.O.Box 144810
Salt Lake City, Utah 84114-4810

Dear Dianne,

Here are the corrected minutes from the Technical Review Committee meeting of November 17, 1993. I have also enclosed the draft minutes of the March 2, 1994 meeting.

The next meeting of the Technical Review Committee is tentatively scheduled for Wednesday, June 8, 1994.

Sincerely,

Deane

Deane H. Zeller
District Manager

Enclosure:
As Stated Above

Minutes of the Technical Review Committee
Meeting of November 17, 1993
Recorded by Philip Allard
Corrected 3/4/94

Attendance:

Committee Members:
- Paul Anderson (PA)
- Craig Forster (CF)
- Jim Kohler (JK)
- Stanley Plaisier (SP)
- Ton Netelbeek (TN)
- Diane Nielson (DN)
- Hugh Coltharp (HC)
- Wally Gwynn (WG)

BLM Representatives:
- Phil Allard (PhA)
- Deane Zeller (DZ)

U.S.G.S. Representatives:
- Jim Mason (JM)
- Lee Case (LC)
- Ken Kipp (KK)
- Geoff Freethey (GF)

The meeting began at 9:30 am at the Salt Lake District conference room.

1. PA opened the meeting and welcomed the USGS and turned the meeting over to JM. JM said that this time KK would go first. LC asked if there were specific areas where the TRC wanted the presentation focused. JK brought up his concern about the '92 K data which show no depletion of K since the data collected by Lines. He thought this presented some difficulties. JM said that the '81 data which could be interpreted to show depletion were actually reflecting seasonal variation due to dilution. JM said that he saw no reason to abandon that conclusion. He said that he was looking at Lines' data and Turk's data. DN said that one of the suggestions that WG had was to look at more than just the K data but also include the other ions. JM said that they do measure the major ions. He said that K and Mg follow the same track and Ca is stable. LC asked if there is some thing that we can do that will help resolve this issue. JK suggested that he get together with JM at some future date and compare their maps and see what can be done. JM said that if the difference was in the analytical data the biggest source of error is the dilution factor. He reported that he had seen some samples where K increased while Mg decreased. They are rerunning these because there is no reasonable mechanism to account for this. He said that for him the only way that one

will see a depletion of K will be to look at the chemistry of the pore fluids. Unfortunately, these data have not been collected in the past. He said that he still felt that the low values recorded in 1981 were the result of a dilution effect. JM and JK agreed to get together. JM said that they can look at this during a long term monitoring program, but this will require a lot of money. LC said that we can get together and report back by next meeting.

2. KK then presented his report. In the last quarter we went to a 3D model and put in permeability distribution. Fluid density from April and September were taken from the Lines report. He derived the permeability distribution from the transmissivity data. He looked at the data between April and September. Earlier they had used uniform permeability in the model but now they are using low permeability at the edges and highest permeabilities in the center and along the southern boundary of the modeled area. With this configuration any little gradient will move water to the south. KK looked at the difference between April and October and there was very little difference, so fluid density is only a second order contributor to the model.

3. The last time they ran the 3D model they were using a 5,000 foot horizontal mesh. PA asked if the range in densities observed over time was the same range as seen in the difference between April and October in the Lines' data? KK said the density does contribute some to the model but not dramatically so. JM said that changes in density over time are caused by variation in the NaCl content. NaCl is staying relatively stable over historic time. KK added that the difference in permeability is caused by aquifer characteristics and not the density of the fluid.

4. KK said that they haven't changed the shape of the modeled area but they have gone to a 2,000 foot horizontal mesh. They have made some changes to the boundary conditions. The east is a leaky boundary with a 4,214 foot head rather than a specified flux. The north boundary is a specified flux. There is a specified pressure on the south boundary of 4,210 feet which is representative of the system when it is in production. At the Silver Island Mountains they are trying a leaky condition with a head of 4,206 based on measurements. The western boundary is a specified pressure of 4,213. The model is only three node layers in the vertical dimension. This represents a 2,000 foot horizontal mesh and a 15 foot vertical mesh. Specified precipitation into the model is one of the most significant inputs into the model.

5. The first water table map generated by the model shows a mound building above land surface and then a large amount of water being dragged into the Silver Island Mountains. A lot of what is seen is a consequence of the variation in permeability. Early runs with uniform permeability didn't show the same build up to the north but did show a similar geometry. The bottom boundary of the model assumes no leakage. They are going to try a leaky boundary on the

base of the model. KK said they have no data to support vertical variation in permeability in the modeled area so they have assumed variation in permeability horizontally but not vertically. KK said that generally permeabilities are highest in the areas where the salt crust is the thickest.

6. KK then showed a 3D drawing of the water table surface. He pointed out areas where the model didn't correspond to the real environment. Presently the model shows out-flow to the east. In all cases they show significant flow out the south boundary of the area because of the ditch and the permeability of the material beneath the highway. WG asked if this was in error. If the high permeability is related to the presence of the salt crust and the salt crust was removed when the highway was built, perhaps the model is overestimating this flow. JM said that only three vertical feet of the aquifer were disturbed by highway construction. The aquifer is a total of twenty feet thick in this area.

7. Major out-flow in the model is to the south and through the production ditches. There is also out-flow to the east and to the Silver Island Mountains. JM said the main drive on the model is precipitation. We haven't yet put in variable infiltration of precipitation. JM suspects that in the environment this is a variable, but it is constant at this point in the model. KK said that when they add solute transport to the model some of the results will change. When one compares the present run of the model to earlier runs you don't see that much change in the output results. The major difference is what is happening to the east.

8. In future runs they will be looking at leakage across the bottom of the model. They are going to look at variability in infiltration and look at changing some of the assumptions used on the north boundary to correct the artificial mound of water in the north. They are also going to compare their simulated data with the elevation data. With the high gradient and high permeability on the southern boundary they will probably still get those high flows to the south. We seem to be confirming Lines' modeling work.

9. WG said that KK had mentioned that he assumed even rainfall across the area. Do we have precipitation coming on faster than the soil can absorb it? JM said yes. CF asked if they are assuming that all precipitation infiltrates. JM said no, they are working with net recharge. We are looking at a steady state model for the production season and a steady state run for the non-production season. We can't handle instantaneous events (single storms). We have taken averages over time. SP said that it looks like you are working with 5% recharge. KK said that that is about right.

10. PA asked how they are handing surface flow from the south to the north. JM said that they haven't put this into the model.

this flow happened in the 92-93 winter and may have happened in the 83 winter. This is an episodic winter condition. PA asked if they were going to consider this as a flux to the system. We haven't put this into the model. We are looking at boundaries and flow during production and non-production, but we are not looking at simulation over time. PA asked if the flow from the south would increase the flux to the surface to greater than what would be predicted from just precipitation. JM said that the ponds occur when there is no pumping. There is no place for this water from the south to go to infiltrate. PA said that there is no ponding in the model. He wondered if the model was under estimating input to the system because of this. JM responded that they are modeling the producing season at this time and ponds do not occur during the production season. KK added that they do plan to run a model for the non-producing season.

11. CF asked if they could give a sketch of the remaining modeling efforts that are still planned. KK said 1) we will add solute transport to the model, 2) we will be evaluating seasonal variation after we have the boundaries more strongly established, 3) we will take a more detailed look at the system vertically, 4) we will compare the model to real water level data when these data are available, and 5) we may also look at a deeper cross-section model if we feel comfortable with the available data. CF asked if they were going to look at transient flow as a component of the solute transport model. KK said yes and they also are going to tie the model to ditch production, elevation and precipitation measurements. The overall goal is to get an overall balance of flow and solute because they don't have the data on point to point velocities. PA asked if they can put salt in the model and take salt out. KK said that this is possible only in a bookkeeping sort of way. JM said that they will have to assume some sort of "average" production year.

12. CF asked if the USGS could draw a linkage between the salt balance and the cause of the apparent loss of salt on the race track. JM said that you have the measured salt loss from Brooks. We can look at the salt balance and see how close this is to 1% per year. 1% per year is about 5,000 acre feet of brine at 1.18 density. If the boundary conditions of the model equate to this then there will be confirmation of the Brooks study. We will be getting a net out-flow and in-flow of salt based on a budget that we are doing. The model will indicate if you are drawing salt from storage or if there is recharge of salt to accommodate the salt balance.

13. PA said that the USGS will be using average conditions in their modeling effort but that he (PA) still has a question as to how the USGS will be handling the extreme condition of water entering the study area as surface flow from the south as a component of the average. JM said that the real problem is that there is so much variation from year to year and season to season.

CF said that the best approach would be to get year by year infiltration data but that this is very difficult to acquire with tritium/helium data and even this may not work. PA said that the pond do have significance from the salt balance point of view. TN said that the salt replacement project would also be causing introduction of brines from the south. Are these brines from the replacement project just going to be recycled through the aquifer? JM said that another question is to what extent is mixing taking place in the aquifer.

14. JK asked if they had taken enough samples of the surface waters this spring and summer to get a feel for when they reached saturation. JM said that they sampled in January and it was saturated by May. Once evaporation starts the pond becomes closer to saturation.

15. JK asked if they had observed any difference to the salt crust this year as compared to previous years. JM then showed an overhead of satellite data and showed 9/92 and 8/93. There was a much larger area of salt crust in the 8/93 photo. It took quite an effort to get the data reduced to the point where they are displayed. CF offered data tapes from late summer '83, '84, and '86. JM said that that would be worth looking at. CF said that the impression that you got was that it wouldn't take much to change the image, so the more pictures you get the better your sense of the variation in the system. JM said that once they have classified the image we can import the data to arc/info so that you can display the data at any scale that you want. We then derived the amount of salt dissolved by the pond. We used the density number that PhA got this fall. We are now working on getting a map of where salt was precipitated. About 2,000,000 tons of salt precipitated before the ponds were totally evaporated. As a minimal estimate 1,000,000 tons of salt were brought to the salt flats from south of the highway this past season.

16. SP asked if the USGS had done an overall mass balance yet. JM said that he will be generating a mass balance to use with the model. LC said that one thing that USGS does is present measured data separately from data generated through modeling to ensure that there is no confusion between the measured data and information generated by modeling.

17. JM then showed an overhead of the classification data from the image. There is an area of the salt flats that shows differently from other areas on the salt flats on one image. He is looking for a hydrological reason for it. At this point it hasn't shown on the '93 image. He feels it may be surface texture. CF suggested that it may be a function of the depth to ground water that may change the optical characteristics of the salt. JM said that there is quite a bit of difference to the texture of the salt this year. He said that this year the hard surface hadn't formed because you didn't have enough evaporation.

18. JM said that in the written quarterly report there is a discussion of the collection of tritium data. JM reported that he had not yet received the results from the lab on all of the samples. The tritium data should help in the understanding of the vertical component of the system. One well at the weather station contradicts the water level data. They found evaporative water that is tritium dead (pre-bomb) indicating an upward gradient. This well is screened from 9 to 14 feet.

19. PA said that the TRC wanted to talk about LC's suggestion for two reports. How does the USGS plan to do this? LC said that this report will be open-filed pending a water supply paper. The open-file will allow it to be on the street sooner. When it is published as a water supply paper Headquarters takes the print cost. The open-file is included in the estimate. The other document is envisioned as a pamphlet or something similar to the one on the Great Salt Lake. LC said that he felt strongly that there needs to be a summary put out, but it will have to cost a little bit more. DN asked if the pamphlet and the open-file would be released at the same time. LC said that the pamphlet would have to have a technical basis. The pamphlet would refer to the open-file so the open-file would probably be released first. LC said that he didn't think that the pamphlet would add to the writing time of the report. LC said that he felt the report was due in FY 94. SP asked when they thought they would have all of their data. JM said that they hoped to have all of their data by the first of the year. The modeling would still be going through April and their best estimate for having the draft report available was the beginning of July. LC said that the process was to have a draft, then supervisor review, then colleague review, then respond to comments. LC said that his goal was to have JM and GF completed with their portion by September 30, 1994. If this happens then a printed copy of the open-file would be available for release by January, 1995.

20. PA said that the TRC would be interested in seeing the draft and commenting. He said that he didn't think that the TRC would want to be a formal colleague review - that would depend on the individual member of the committee and what their particular interests may be. LC said that every written comment must be responded to and explained. CF suggested that the BLM should guide the TRC through this process. TN suggested that oral comments could be helpful but take less time then written comments. LC said that his policy was that there will be a written response to all written comments but oral comments may not require a written response. TN said that the TRC doesn't have a defined responsibility for the report. LC said that they need two colleague reviewers, one from within the District (USGS organization) and preferably one from outside of Utah. Perhaps the data and its' use. It was decided that the next meeting of the TRC would be held on February 2, 1994 at the USGS office.

Minutes of the Technical Review Committee
Meeting of March 3, 1994
Recorded by Philip Allard

Attendance:

Committee Members:
- Paul Anderson (PA)
- Craig Forster (CF)
- Stanley Plaisier (SP)
- Wally Gwynn (WG)

BLM Representatives:
- Phil Allard (PhA)
- Carla Garrison (CG)
- Willie Robinson (WR)
- Clark Hansen (CH)

U.S.G.S. Representatives:
- Jim Mason (JM)
- Joe Gates (JG)
- Geoff Freethey (GF)
- Ken Kipp (KK)
- Lynette Brooks (LB)

Guests:
- Gary Allen (GA) — member of the public
- Lloyd Austin (LA) — Department of Natural Resource, Division of Water Resources

The meeting was held at the office of the Water Resources Division, U.S.G.S., Salt Lake District. The meeting began at 9:05 am.

1. JM began by discussing his quarterly report. He reported that they had received elevation and location data on the monitoring wells from Cadastral Survey of the BLM on 2/2/94. They were hoping to contour water levels using ARC/INFO but found the software had difficulty handling boundary conditions so they will be required to hand contour the water level data, and then digitize the contour map. They began with the 8/93 data. Since correcting the water level data to average density, the gradient to the northwest is only 2 feet rather that 6 or 7 feet. They haven't looked to see if this change was solely related to the density correction or if elevation data on the well casing had changed. SP asked if that same change was seen in every data set. JM said that they have only contoured the August data but they did look at the 4/93 data run and the gradient was still present. It could be a gradient created by vegetation.

2. It appears that there was no water drawn from the collection ditch except from the lower end of the ditch. SP said that the

annual production season for Reilly runs from 9-1 through 8-31. JM said the August measurement was right after the heavy rain in July. He also expressed concern that if Rally pumped year round there would only be production times, there will not be non-production times. This would effect the assumptions used in the model. JM said there still is a significant gradient to the south under the freeway of about 8 feet per mile. It begins just north of the freeway and gets very steep next to the collection ditch. PA asked which collection ditch. SP said that this a new collection ditch about 2 or 3 years old. This ditch parallels I-80 on the south. JM said that the level in the ditch is about 4200 feet corrected to an average density of 1.17. The lowest piezometer is 4,203. SP asked if any cross-sections had been plotted. JM said no. CF asked if there was a change in permeability under the freeway or if the ditch really was a sink. JM said that he really didn't think it was permeability issue under the freeway. He said that he didn't think the railroad had lowered permeability either. The gradient seems related to the presence of the ditch. The gradient is seen north of the freeway and south of the freeway. CF said that he was comfortable with the ditch as a sink, he just wondered if there were superimposed effects present.

3. JM said that in his report he gave a summary of the satellite data study.

4. JM said he had met with Jim Kohler to talk about KCl depletion. They concluded they could see some depletion in the north part of the salt crust area. There wasn't any depletion seen to the south. This was based on comparing '76 data to '92 data. Kohler had been including other data and JM said that the '81 data set is not representative because it was taken at a different time of year (spring rather than late summer). PA asked why JM said the concentration "might" have changed. JM replied that he was hedging a bit. He questioned if the interpretation could be an artifact of sampling intensity. They are taking the data, plotting it, taking the difference between the two data sets and contouring the difference. SP asked if the analyses were done by the same lab using similar techniques. JM said that they were done by the same lab, but that techniques have changed over time. For example, QA/QC processes have been greatly improved. SP then asked how many of the wells were the same well. JM said that quite a few of the wells were the same so they are not comparing an area merely on a general basis, but are seeing changes on a well by well basis. CF asked if it was a large change or a change so small that it could be considered analytical noise. JM said that the difference was an absolute change in the range of .3% to .4%. This is not a change of .3% of the total, but rather a change of .3% from an original measurement of about 1%. JG asked if the significance of this was that it indicated that KCl was being produced from beneath the salt crust? JM said that this could be the case or it could

indicate a change in the rate of diffusion of KCl from the pore fluids, down to the south there could be a greater amount of infiltration than there is in the north. The winter pond formation is also more common to the south. JM also said that the new salt crust formed near the production ditches is still present and has not dissolved.

5. JM said that they had gotten all the tritium data back. The tritium data from the nested wells on the west side of the alluvial fan aquifer don't support the notion of direct recharge to the alluvial fan from rapid infiltration through piping. All five samples showed tritium values of less than detection or pre-bomb water. CF said that he recalled a cross section of tritium values and asked if the USGS had filled this in with shallower values. JM said that they had and referred the group to the map showing tritium values in the quarterly report. The values from the shallower wells are the upper values. CF said that there are still values that show current, not pre-bomb water. JM said that the only explanation he had was that there is local recharge. The highest values are near the ditch so you are not getting infiltration from the surface near the ditch. The tritium data contradict the head data in the center of the salt crust. The head data indicate a downward gradient and the tritium data indicate an upward gradient. CF said that the head data show present day conditions. The tritium data would tend to indicate conditions from up to twenty years ago, and we don't have old head data.

6. SP asked how the USGS felt about the schedule for data reduction. JM said that they were not on schedule, but that they were of the opinion that the basic data report would be out by late spring. There is still a lot to do and they have just recently received the BLM elevation and location data on the wells.

KK then presented his material.

7. Since the last meeting in late November he reported that they have further refined the mesh for the 3-D model. The mesh now has nodes 1,000 feet apart horizontally with areas near the ditch having nodes 500 feet apart horizontally. Vertically, there are now 6 layers of nodes over 30 feet. This is five feet apart. This gives the model 33,000 nodes of which 27,000 are active. The system doesn't go to steady state because it takes several years to get to steady state and neither the production season nor the recovery season last more than a year. Neither of these conditions will drive the model to steady state. Initial conditions then determine the model results. Initial conditions are assumed to be water at the surface with no flow. It takes 2 to 4 hours to run just the flow model without the solute transport component with variable density fields.

8. KK reported that they had adjusted the boundary conditions. Leaky boundaries are assumed along the north, east, and western boundaries. Potential or pressure field conditions are assumed at the south boundary. The permeability and the leaky thickness are varied along the edges. The potential difference is then shown in the model region. Leakage is effectively high. JM pointed out that the permeability distribution was derived from the transmissivities from the lines report which were similar to those reported by Turk. PA asked about the rational of having the width of the leaky boundaries vary. KK said that they started with a width of 1000 feet all around the model but made it thinner in some areas. The values on the west came from data from the wells near the boundary on the west. JM said that boundary conditions are controlling how things will be calculated. Using the gradient information they designed the boundaries to reflect the data. KK said that they had thought it important to let the heads establish themselves along the boundary. For the other boundaries (on the south of the model regions) they specified a head. They specified 4,208 feet for the head in the federal lease ditches to assume production conditions. JM said that they had to assume an average value because Rally does not have a constant production rate. In order to get the model to run they had to assume an average value. KK said that using the elevation data they were able to use a piece wise variation in the southern boundary. The ditch level was assumed at 4,200 feet and stepped up to 4,206, 4,210, 4,212, and 4,218.

9. KK said that in cross section they are specifying an evaporation flux for the summer season. At the base of the model they established a leaky boundary that was 30 feet thick. This was derived from an average of five wells of 63 feet depth. Presently the model is showing that there is leakage from shallow brine aquifer to the lower aquifer. This could change when they get to the variable density solute transport model.

10. The results of these model runs is similar to previous runs of the model showing flow through the federal lease ditches and the south ditch. The first two figures in the quarterly report show no evaporation. Then they ran the model with evaporation using Lines evaporation data (.0035 inches/day) and Pilot Valley evaporation data. The Pilot Valley evaporation data generated a 2.5 to 3 foot lowering of the water table causing a lot more infiltration along the edges of the model. This run of the model is giving a response that they are not observing. CF suggested that they may be using fresh water evaporation data from Pilot Valley. PA asked if the data assumes that you have a liquid phase exposed at the surface. Perhaps there is a difference in evaporation of water that is contained in the crust from the evaporation of free water on the surface of the salt crust. KK said that what they need is a good estimate of average evaporation. Also they see outflow from the north, south and

bottom of the model in the no evaporation situation.

11. CF asked if they were to run the model for more than one season would they get away from the initial conditions. KK said that one could do that and perhaps reach a periodic steady state. JM said that they could do that but they would have to make some assumptions as to what the seasonal variation was. KK said that when using Lines evaporation data total outflow due to evaporation was much greater than other outflow so the model seems to be quite sensitive to the assumed rate of evaporation. This run also gives upward leakage from the bottom of the model, but much of this is an artifact of the calculations. The lowermost nodes along the edge of the model have a component of flow horizontally and also a component of flow vertically. The majority of nodes at the base of the model show flow from the brine aquifer to the lower aquifer. PA pointed out that this also shows some of the assumed boundary conditions. KK said that when one used the higher estimate of evaporation you got in-flow through the federal lease ditch.

12. KK said that the next set of simulations will use a measured initial condition and go to the variable density effects. The data used to construct the initial condition are quite sparse.

13. SP asked what the longest simulation time used, was it longer than six months? KK said yes. He did get to steady state on several runs early in the model. He said that 40 to 50 years in the model were needed to reach steady stat. KK pointed out that because the environment is not a steady state environment, such model results were of little utility. He pointed out that the environment gives periodic draw down, but doesn't achieve full recovery before draw down begins again.

14. LB then gave a presentation on the work done with satellite imagery. The USGS had acquired an image from the Landsat Thematic mapper. This image was taken 8/9/93. They started the analysis using ELAS (?) software and then took the image into ARC/INFO. They began without classifying the data to see if there were any obvious relationships with the data and depth to groundwater. This didn't reveal anything so they classified the image based primarily on band 2 and band 7. They used a random classification and then tried a supervised classification. In the supervised classification they took an area where they had known characteristics and then used this area as the basis for classifying similar areas. They were able to determine the area of salt crust but they were unable to determine the depth to ground water using the satellite. They have not yet completed the analysis of salt migration. They have defined the surface area change in salt crust, but they need to map the thickness change data and combine this with the area data to generate volumes.

Post meeting

1. SP moved that the existing officers of the TRC have their terms extended through the present term of the charter. WG seconded the motion and it passed unanimously.

2. PhA announced that Ton Netelbeek had resigned from the TRC.

3. SP moved to accept the corrected minutes of the meeting of November 17, 1993. The correction related to item 7 on page three. This item was rewritten for clarity. This motion carried.

4. It was decided that the next meeting would be held at 9:00 am on June 8, 1994. The TRC hopes that the basic data report would be available by that time and that the water levels would have been contoured. They will also need to address the end of the charter at that meeting.